

R E V I E W S .

ART. XI. *General Therapeutics, or Principles of Medical Practice; with tables of the chief remedial agents and their preparations, and of the different poisons and their antidotes.* By ROBLEY DUNGLISON, M. D., Professor of Therapeutics, Materia Medica, Hygiene, and Medical Jurisprudence in the University of Maryland; formerly Professor of Physiology, Pathology, Obstetrics, and Medical Jurisprudence in the University of Virginia; one of the Physicians to the Baltimore Infirmary, etc. Philadelphia: Carey, Lea & Blanchard. 1836. pp. 580, 8vo.

The successful compilation of an elementary work, on an extensive and constantly progressive science, is always a very difficult task. Such a work ought to embrace, within limits of reasonable extent, in well defined and strongly marked outline, the great boundaries and the minor subdivisions of the whole subject treated of. It should present this subject to the naked eye of the learner, in its natural colours and proportions, without the intervention by the author of any refracting or distorting media of his own composition. It should tell the truth, and nothing but the truth, although it may be unable to tell the whole truth. It requires, on the part of its author, a rare combination of high and various qualities. He must be familiar with the whole past history, with the actual condition, and with the future prospects of the science which he teaches. He must see, not in dim and shadowy vision, but in the sober certainty of clear and open light, its multiform relations to its kindred sciences. He must understand its errors, past and present, as thoroughly as he does its truths. Then he must have a sound mind, with its powers harmoniously balanced, if possible; and if not in perfect equipoise, with the cautious and doubting tendencies preponderant. There must be no crotchets in his brain; he must be no keeper or rider of hobby-horses. With the power of fully comprehending his subject; of distinctly distinguishing, in his own consciousness, the true from the false, and the doubtful from both; he must, furthermore, be master of the art of presenting the clear and luminous conceptions of his own mind, clearly and luminously, to that of his reader. He should have the excellent faculty of adapting his form of words to the nature of his subject, seeking always the ornament of simplicity, directness and order; eschewing meretricious finery and pedantic stateliness of style, and avoiding also tameness, sleepiness and obscurity. Especially should the compiler of an elementary work, on any department of practical medicine, be possessed, in addition to the foregoing qualifications,

of a healthful and sensitive conscience. He should be no respecter of persons. He should be a medical cosmopolite, with no party prejudices or local attachments. He should see and feel the nature and extent of the responsibilities under which he acts. He comes forward as the official oracle and teacher of the young physician. His readers are chiefly from amongst this portion of the profession. They imbibe from him their doctrines, they follow his precepts; and since no less than the lives and health of their patients are the things at issue, it is all-important that the doctrines should be sound and the precepts true which are to be the basis of their practical conduct.

There are some reasons why a correct and comprehensive elementary treatise on the materia medica and therapeutics should be more difficult of execution than a similar work on most of the other branches of medicine. The improvement of the materia medica and of therapeutics has not kept pace with that of the other branches. They have been comparatively neglected. There has been less of rigorous, systematic observation devoted to them than to other portions of our science. The leading minds in our profession during the present century have been directed more particularly to pathology, physiology, &c., and to certain limited portions of therapeutics and materia medica connected with certain symptoms of pathology and practice. It is vastly more difficult than has generally been supposed, or than most of us are even now willing to admit or believe, to ascertain the actual and precise value of any given article or course of treatment, even in any one given disease; and the evidence upon which this value rests is in very many important instances exceedingly slight and doubtful. There is less positive knowledge on this subject than on most others in medicine. For these reasons, and for others which might easily be given, we repeat, that the satisfactory execution of a work such as we are now speaking of, must be a very difficult matter. That a work of this character was much needed amongst us before the publication of the book, the title of which stands at the head of this article, we are well aware; we think it as much needed at present as it ever was. The grounds of this belief will appear in the course of the following pages.

The first chapter of Dr. Dunglison's treatise is devoted to "General Principles," in which the following subjects, of very various character, are more or less fully discussed, to wit:—Therapeutics defined; instinctive action of recuperation; importance of bearing it in mind in the treatment of disease; cure by sympathy; expectant medicine; efforts of nature; crises; medical experience; science of medicine demonstrative; pre-eminence of therapeutics; therapeutical indications vary with medical theories; necessity of discovering the pathological lesion; rational therapeutics founded on rigid physiologico-pathological deduction; importance of discovering the cause of the lesion; etiology obscure.

It is not our purpose to make the present notice consist exclusively of a running commentary on the successive subdivisions of Dr. Dunglison's book; neither do we intend to make the title of the work a

mere text for a therapeutical sermon of our own—an occasion solely for the setting forth of our own individual views or opinions on this branch of medical science. It would be quite impossible, from the nature of the work itself, to present our readers with any thing like a full and profitable abstract or summary of its contents, in the form of an analytical review, without far exceeding the limits suited to a paper like the present. But while we shall endeavour to furnish our readers with a general knowledge of the work under notice, we shall also be obliged, in order that our criticisms of certain of the author's leading doctrines and opinions may be rendered intelligible, to state somewhat fully, and as explicitly as we are able to do, our own views on these subjects.

In the study of any one separate portion of a great and complex subject, a first and most important inquiry must refer to the nature and extent of the relations between this and the other several portions of the entire subject. The science of medicine, taken as a vast, comprehensive, and integral whole, is made up of many other sciences; each of which occupies its own appropriate section of the large and perfect circle, wears its own individual features, and accomplishes its own peculiar ends; and each of which is, also, more or less intimately, and by relations more or less direct, connected with all the rest. Certainly, it is important, in order to understand with any satisfactory degree of clearness the chief science itself, that we should have a correct conception of the mutual dependencies, and the bearings upon each other, of the subordinate parts of which it is composed. The only relations, however, which it is our intention to notice here are those existing between therapeutics and pathology. The end of therapeutics—its sole, exclusive end—is the removal or mitigation of disease; and for this or some other reason, it seems to have become an axiom in medicine, that therapeutical indications must always be deduced, directly and immediately, from pathology. So, Dr. Dunghison, in his preliminary chapter, following in the old track, which has been trodden by all the pathologico-therapeutical systematists, asserts and maintains the same principle. "In all these cases," he says, "as in every other, rational therapeutics must be founded on rigid physiologico-pathological deduction."

We shall take some pains to show that this immediate and intimate dependence of therapeutics upon pathology does not exist. That there is a near and close relationship between these two capital departments of medical science, we readily admit; but we do not believe its nature to be such as it is generally stated to be. Therapeutics does not flow directly from pathology. The word *deduction* is not the link which binds the former to the latter. No therapeutical indication can be inferred, directly, and without the intervention of some other aid or agency, from any known pathological condition. No remedial measures can be based, originally and exclusively, on a simple knowledge, however exact and certain, of the seat, degree, or nature of a morbid action. This, at any rate, is the general rule. We

might, indeed, in a few cases, independent of any aid derived from experience, infer with a reasonable degree of probability, the appropriate remedy. But these cases, on close examination, would be found to be composed mostly of lesions where the indication consisted merely in the removal of an obvious cause. The first man, who ever suffered with a splinter of wood under his finger-nail, might very properly come to the conclusion, that the withdrawal of the splinter would be the first and surest means of removing the pain, although this therapeutical indication had not been taught him by his own experience or by that of others. We may suppose, that pathology had explained to us the condition of an organ suffering with recent acute inflammation. Looking at the undue activity of the circulation, and the great distension of the blood-vessels, we might, perhaps, be led by simple, unaided, *à priori* reasoning to the abstraction of a part of the superabundant blood. So we might, in the same manner, regarding the unusual heat of the part, where this could be ascertained, resort to the application of cold. But with these, and some few other analogous exceptions, we do not, and we cannot, deduce our therapeutics from our pathology. The case seems to us almost too plain to be argued, and yet nothing is so common as the contrary assertion. We are constantly told that the great essential prerequisite to the successful and rational application of remedies, is a knowledge of the seat and nature of disease; that the latter once clearly settled, the former follows as a matter of course; that the one *depends upon*, and *rises out of*, the other. The necessity of this knowledge, so far as it is attainable, we admit; but we deny the soundness of the reasons on which this necessity is usually placed.

Talk about it as we may, therapeutics is an art resting solely on rational and philosophical empiricism. We are often, it is very true, obliged to resort to remedial measures in the treatment of diseases, the seat, nature, and complications of which are more or less novel or obscure; which may, indeed, be such as have never before been manifested in the living organization. Here we are, to a certain extent, driven to the shelter of analogy and to *à priori* reasoning. So far as the pathology in some of its elements, or in their aggregate combination, resembles other morbid states with the treatment of which we are already familiar by direct experiment, so far we transfer, rightly enough, the same views and the same treatment to the new malady. But let us not deceive ourselves even here. Our *à priori* reasoning is itself acting under the guidance, uncertain though it be, and walking in the light, feeble though it be, of experience; and the first steps of our treatment are wholly tentative. There may exist some new, and as we had supposed, perhaps, an unimportant and subordinate element in the series of morbid phenomena, constituting the new disease, the presence of which may render the treatment *indicated* by analogy entirely useless, or worse than useless, and experiment may finally demonstrate the necessity of an opposite mode of management.

We have alluded to the stress which the systematic therapeutists of the day put upon a knowledge of the *nature* of the morbid affection,

as preliminary to its successful treatment. But what do we know of the nature of disease? What two leading pathologists are agreed on this single point? What is inflammation, that every day disease, always before our eyes and under our hands? Is it the same *ipse morbus* in every organ, in every tissue, during its successive periods, and under the all-controlling agency of varying epidemic influences? Does any knowledge, that we may profess to have of its nature, direct us in its management? On the contrary, do we not often find it yielding under certain conditions to one course of treatment, and under certain other conditions, to an apparently opposite one? Is phlegmonous erysipelas arrested by the same means that will cure acute inflammation of the knee-joint? Will a solution of nitrate of silver, dropped into the eye, relieve a conjunctivitis of six hours standing, arising from the presence of a grain of sand? Will the purulent ophthalmia, where there is less redness, perhaps, less local suffering and less constitutional disturbance than in the slight affection, just mentioned, give way to a few leeches and a mucilaginous lotion? Our knowledge of the most efficient treatment of inflammation is the single, direct result of naked, unaided experience. Our knowledge of the appropriate treatment of the almost infinite forms and modifications of this morbid state is, also, the simple, direct result of naked, unaided experience. Having ascertained, by experimental researches, the most effectual method of controlling inflammation, when seated in a certain tissue or organ, or occurring under any given circumstances, the logic of analogy might have led us toward the true treatment of a similar morbid condition, existing under more or less similar circumstances; but this same logic might, also, have led us to the worst possible treatment. And this is true of all diseases. Who ascertained and demonstrated the relationship between mercury and syphilis? Was it scholastic PHILOSOPHY, now in robe and slippers, meditating apart and alone, in her quiet closet; now aloft on her airy wings, piling up and adorning her gossamer castles in the sky; or was it EMPIRICISM, that humbler handmaid of our beneficent art, with her senses always awake and her fancy asleep, or obedient ever to the voice of her reason, and with no *muscae volitantes* swimming before either her bodily or her mental vision? Could PATHOLOGY, poring over the essence of the venereal virus, and studying the nature of the morbid action which its presence excites, even if she could have ascertained these things a thousand fold more perfectly than she has yet done, have ever led us, *by induction*, to the appropriate antidote and remedy? Whence came our knowledge of the action of cinchona in intermittent fever? Wherein consists the resemblance between this substance and arsenic, that should have led us by *à priori* reasoning to the use of the latter in the cure of the same disease? How is the most successful treatment of typhus fever to be ascertained? Is it by *deductions* from its *pathology*? The morbid alterations in this disease, their successions and dependencies, have been very thoroughly investigated. The condition of the suffering organs is very satisfactorily known. The relations between the external manifestations, or

symptoms, and the organic lesions, is pretty well settled. The *natural* history of the disease is in a fair way of being fully completed. Its specific characters are made out, so that it is easily and certainly recognised. One of the essential elements of its pathology consists in a morbid alteration of the glandular bodies of the small intestines, and of the mesentery; an alteration which we call inflammatory. Knowing this, can we thence *infer* the most appropriate treatment? Most certainly, we cannot. The pathology of true typhus is now as fully and as positively known, perhaps, as that of any other disease, so complicated in its pathology as this. Can we now *deduce* its proper treatment from its admitted pathology? Most certainly, we cannot. This treatment is yet to be discovered. At the outset of our therapeutical researches here, we might, indeed, if we had no better guides, listen to the suggestions of analogy. But let us beware of that logic which teaches us to place implicit faith in the seductive promises of an over-confident, generalizing, and systematizing philosophy. The question, which we are called upon to solve, in the case of this disease, and of all other diseases, is, not whether this remedy or this method is a rational or a philosophical one; not whether it is in accordance with our notions of the pathology of the disease, but whether it is or is not a successful one. What matters it whether our treatment be antiphlogistic or incendiary, expectant or perturbing, if, by it, we save our patients from suffering and death?

We shall, probably, be met here with the old objection, founded on the difficulty of applying this test. But the difficulty of its application does not prove that the test itself is not the true one. Dr. Dunglison says,

"It is obvious, that, *cæteris paribus*, therapeutics should be the touchstone of medical skill: the number of cures ought to decide the qualifications of the practitioner; but it so extremely difficult—nay, impossible—to estimate all the deranging influences;—so many modifying circumstances are perpetually occurring, that we cannot decide that any two cases are precisely identical. Hence we can never judge of the comparative success of different practitioners, on which so much stress is placed—and placed erroneously—by the public. Owing to these difficulties also, we have such a diversity of sentiment regarding the treatment of the same affection."

We are not prepared to assert that the *public* are justified in judging of the merits of a practitioner by his apparent success. So far, indeed, as the opinion contained in the above extract refers to the public, it is undoubtedly correct, and this for plain and obvious reasons. But we do assert, that the real value of a remedy, or of a method of treatment, in any given disease, and of all remedies and of all methods of treatment, in all diseases, must be judged by their actual and relative success, and by this alone. The "deranging influences," and the "modifying circumstances," which are perpetually occurring, will, to be sure, prevent us from deciding that any two cases are precisely alike. We may be very sure, that no two cases ever are precisely identical. But still these "influences," and these "circumstances," constituting, as they do, the difference between different cases, are none the less susceptible of being studied and *appreciated*.

It is this very appreciation upon which, in part at least, our diagnosis rests. Their importance as disturbing causes can be ascertained and estimated. And it must be ascertained before the natural history of the disease is fully made out. They constitute as much a part and parcel of this history as do the most essential morbid alterations, or the most prominent and invariable symptoms.

We are quite willing to go as far as Dr. Dunglison goes in insisting on a thorough knowledge of the seat and nature of the morbid lesion, so far as these can be ascertained, as an indispensable pre-requisite to an enlightened and successful practice. But this preliminary knowledge is essential only as an element in our diagnosis; it is of service only so far as it helps us to recognise the individual morbid condition with which we have to deal, and so to identify it, more or less perfectly, with the same condition whose relation to certain remedies and modes of management experience has already pointed out to us.

It is a little remarkable, that a work which founds so much of its claim to excellence and authority on the importance which it gives to pathological knowledge as the only safe basis of therapeutical indications, and which is the production, too, of a distinguished and experienced compiler—of a man who must be supposed to be even, at least, with the progress of medical science—should exhibit, so frequently as it does, evidences of an erroneous or doubtful pathology. On page 90 we have the following explicit and unqualified statement: “*Phthisis pulmonalis* is a chronic inflammation of these organs, (the lungs,) ending in suppuration and disorganization.” At page 109 it is asserted that laughter is a *not unfrequent* exciting cause of apoplexy. At pages 186 and 187, Dr. Dunglison speaks of a patient of his own, affected with hæmatemesis and *dropsy of the lower belly*. In some observations on the use of emetics in whooping-cough and asthma, page 221, Dr. D. says:

“Both these diseases are dependent upon a morbid condition of the nerves of the respiratory organs—the pneumo-gastric especially—which modifies the contractility of the muscular fibres, that surround the minute bronchial ramifications; and this state of the nerves is generally perhaps connected with more or less morbid derangement of the parts of the cerebro-spinal axis, whence the nerves originate.”

It must be gratifying intelligence to the laborious pathologists of the old world, as well as to those of the new, that the pathological problem of these two affections, especially of the latter, has been, at length, after so much investigation, and amid such widely differing opinions, so satisfactorily and positively solved. We might cite many other instances similar to these, exhibiting either loose views of pathology, or a reprehensible carelessness of phraseology in stating them.

We have thought it proper to state our views on this subject at some length, because we believe them to be not only true, but also of some practical importance; and because the author of the work before us inculcates so earnestly doctrines of an opposite character.

It is difficult to say whether medicine has suffered most from a

partial and one-sided observation, or from premature and hypothetical generalizing—from false facts or from false reasoning. The latter is the legitimate offspring of the former; and although each may very well exist without the aid and presence of the other, they are very commonly found together. We deceive ourselves when we boast, as we are so much in the habit of doing, of our discipleship to the true Baconian philosophy—of our faithfulness to the rules of cautious, impartial observation, and to the strict principles of an upright, a rigorous, and a single-hearted logic. Before our science can take its proper place by the side of the other sciences, and confer that benefit on humanity which it was intended, and which it is able to confer, it must endeavour to become in truth what it has, indeed, long professed and claimed to be, a demonstrative science. Its cultivators must begin to practice what they have so long been preaching. The standard writers of the present day, on therapeutics and materia medica, are constantly indulging in what they may deem very philosophical, but in what seems to us very fanciful, *explanations* of the intimate and peculiar action of medicines on the living tissues with which they come in contact, or which they may affect more remotely. More pages are often taken up with elaborate disquisitions on the hidden, mysterious, and utterly unascertainable *modus operandi* of a remedy or a class of remedies, than are given to the therapeutical properties and uses themselves of the article or the class.

Dr. Dunglison's book, like nearly all others upon the same subject, is overburdened with these attempts to explain this precise and intimate *modus operandi* of medicines. The action and operation and effect of every article must be accounted for pathologically, physiologically, philosophically, rationally. It is astonishing to witness the pertinacity, activity, and ingenuity of this "*detestable mania for explanation*," as the authors of a recent and excellent French work on materia medica and therapeutics, call it. It is not enough that any given medicine or mode of treatment *cures*. This knowledge would be mere empiricism, unworthy altogether of the scientific physician. We must know *how* it cures, and *why* it cures; and unless these things are made out, we are bound to believe that it does not cure at all; that we have been mistaken, and that the two circumstances of the use of the remedy and the cure, which simple observation had taught us sustained to each other the relation of cause and effect, must have been only accidentally so connected. If any one circumstance exhibits more strikingly than another the folly and absurdity of this passion, it is that of the multifarious and contradictory explanations that are continually and successively invented and maintained. There is hardly a page of Dr. Dunglison's book which does not contain more or less paper, spoiled, and worse than spoiled, by magisterial and confident statements of these *hows* and *whys* and *wherefores* of pathology and therapeutics. Does any one doubt this? Then let him study the book for the purpose of settling this particular point; and when he comes upon an explanation, let him inquire whether it is anything more than a conjecture. We will illustrate our remarks

by one or two examples, taken almost at random. At page 81, the author, in speaking of the difficulty with which absorption is accomplished in malignant cholera, and in other diseases of the gastro-intestinal mucous surface, says, "It is on this account, also, that there is frequently so much difficulty in affecting gastro-enteritic patients with mercury." The reason here given, so confidently, and so much as a matter of course, why the system is not affected by the mercury, may be the true one, or it may not be. Do we really know anything beyond the fact, that in certain morbid states of the body it is difficult or impossible to produce the specific effects of mercury? Certainly we do not. Again, antispasmodics are such, according to Dr. D., only through their revellent or derivative operation. He says, at page 382, that direct antispasmodics are revellents only, "acting by virtue of the new impression they make on the gustatory or gastric nerves, and thus deriving from the inordinate action going on in some other portion of the nervous system." Now, as we said before, and as we readily admit in all these cases, this may be true; but we say, also, in this case, and in all the others of a similar kind, that it may be false; and we say, furthermore, both in this case and in the others, that the chances are in favour of the latter contingency. Is it at all unreasonable, or unscientific, or improbable, to suppose that there are substances which allay spasmodic action by a direct, immediate operation on the unduly and irregularly excited organs and tissues? Are not observation, analogy and sound reasoning all in favour of this supposition? If the acknowledged antispasmodics act only by their revulsive agencies, it must even then be admitted that there is something peculiar and specific in the *nature* of the revulsion which they occasion, so that, after all, this hypothesis does nothing towards simplifying our notions of their *modus operandi*. We do not wish to be understood as asserting, positively, that there are any such medicines as simple, direct, absolute antispasmodics, controlling inordinate and irregular muscular contraction by an immediate influence on the nerves going to the disturbed muscles, and without the intervention of any other modification or influence upon any other tissue. We do not say this for the simple reason, to us always a good and sufficient one, that there is no conclusive evidence that such is the case. We only say, that this proposition is quite as reasonable in itself and quite as likely to be true as that of Dr. Dunglison.

Passing over our author's second chapter, which is devoted to a consideration of the circumstances that modify the therapeutical indications, such as age, sex, original conformation, habit, climate, &c., we shall make the third chapter the occasion of a few further remarks on this disposition to explain and interpret the philosophy of therapeutics, and on the fondness for generalizing to which we have already alluded. The chapter treats of the modes of action of medicines, and of their classification. Dr. D. says that the action of medicines is either local and direct, or general through local influence. The general action, according to his arrangement, is threefold, to wit: by means of the nerves, by absorption, and by revulsion. Now, we hate

hypercriticism and mere verbal fault-finding with a good author as heartily as anybody can; and we are not about to object to this division of Dr. Dunglison's, because we think it worse than various other analogous arrangements made by other therapists. It may be better than any of its predecessors, and it is not our intention to attempt to settle the relative merits of the different classifications or their respective claims to superiority. We object to this, and to all, as imperfect, hypothetical, erroneous. Does this objection need developement or illustration? Is it founded in reason or truth? Let us see.

In the first place, then, we admit that there is some foundation in nature for the primary or classic division of remedies into local and general. But even here the boundary line between the two classes is oftentimes shadowy and undefined. A refrigerant lotion, or an emollient, may act locally when applied to a local inflammation. But even in this simple case the localization is topographical merely. The action of the simple remedy may be very complex. The nervous tissue, the vascular, the dermoid, the cellular and the muscular may each receive, individually, a part of the new impression. In regard to the great majority of active remedies, administered internally, what grounds have we for fixing with any degree of confidence the precise extent of their agency? Dr. Dunglison says that certain remedies act through the nervous system, others through absorption, and others by revulsion. All this may be true, so far as it goes, but there is no evidence that it covers the whole ground; and inasmuch as there is not, it is essentially and radically wrong. It is very probable, to be sure, that prussic acid, for instance, acts primarily, and it may be, exclusively, on the nervous system. The rapidity of its operation seems inexplicable on any other theory. It is very certain too, that many substances are absorbed; that they enter into the mass of circulating fluids, red and white. But what do we know about this beyond the simple fact that it is so? The substance is in this way brought into direct relation to the living fibre, but how it acts on this fibre we do not know. The nature of the impression made by it is a mystery. The third general mode of action is by revulsion. By this is meant, that irritating substances, applied to certain parts or tissues of the body, will, under certain circumstances, diminish or remove morbid actions going on in certain other parts or tissues of the body. And is not this the sum total of our knowledge on the subject? It is fashionable, we know, and it is considered *scientific* and rational to philosophize on the matter, and to explain *how* and *why* it is that this effect is produced. The *rationale* of this medication has been sought for and found in the old and oft repeated axiom, that two morbid actions cannot very well go on in the system together. But how far is this from being even generally true. It is now well settled that, in some diseases, in typhus for example, there is a strong disposition in very many of the different organs and tissues to take on acute inflammatory disease, one after another, or simultaneously even; the intensity of each being increased rather than diminished by the spreading of the morbid action over different points of the economy. How do

we know that mercury, taken as an alterative, acts on this principle of exciting a new disease incompatible with the old one for the cure of which it is used? Is the revulsive *modus operandi* in this case any thing but absolute gratuitous assumption? Is it any thing but whole cloth hypothesis? There are certain morbid conditions which mercury generally removes, and we have just as much reason for saying that the mercury acts directly on these morbid conditions, as we have for saying that it excites a new disease in the system, and thus drives out the old one. Let us be contented with asserting what we really know, which is simply this—that the mercury removes or mitigates the disease. Let us ascertain, with all possible accuracy, by long continued, varied, honest, careful observation, every circumstance which exerts a favourable or an unfavourable influence on the remedial action of the article, and therewith let us rest satisfied. In this, and in all other cases, the only legitimate object of investigation consists of the appreciable phenomena growing out of the relations existing between the remedy and the diseased human body. When we have ascertained the laws which govern these phenomena, we know all that we can know, and all that we need to know.

If fanciful and speculative men choose to amuse themselves, or the rest of the world, with their conjectures and explanations, surely we have no objection. They may find the occupation as pleasant and as profitable as any other species of air-castle building; but let them not dignify this guess-work with the misnomer of inductive philosophy; and let them cease to think that they are doing any thing to advance the utility of medicine as an art; the object of which is the mitigation of human suffering and the lengthening out of the term of human life.

The latter part of the third chapter is taken up with the subject of classification. Our author, as every new writer on therapeutics is in duty bound to do, makes some strictures on the classifications of his predecessors, and adopts a new one of his own. We do not wish to indulge in any further criticisms on Dr. Dunglison's arrangement of his classes. Next perhaps in unprofitableness to the building up of these systems, is the labour of pulling them down. The greatest objection we have to our author's, is merely that it is a new one, and that we have thus an addition, utterly useless, made to a catalogue already too long. The old arrangement, based on simple observation and common sense, is the best that can be adopted in the present state of therapeutical science. This arrangement is founded on the most prominent and obvious action upon the body of the different articles of the *materia medica*. When a substance is found by experience to possess the property of directly allaying irregular spasmodic actions of the muscular fibre, let it be called an antispasmodic, and let all other substances endowed with a similar property be arranged in the same family group. Let the same thing be done with emetics, tonics, cathartics, narcotics, &c. Let certain other articles, such as cinchona, mercury, arsenic, iodine, &c., the administration of which, in moderate but repeated quantities, is attended by slow, gradual and

peculiar changes in the economy, be denominated, provisionally, at least, alteratives or specifics.

And here, lest Dr. Dunglison, or any of our readers should be alarmed at our use and sanction of the concluding word of the last sentence, as they may already have been by our use of the word empiricism, we will state, as concisely as we can, what seems to us to be the truth in regard to the specific action and character of the articles of the *materia medica*. With these remarks we shall conclude our general observations upon those leading doctrines of the book before us, about which we venture to dissent from the opinions of its author.

It is quite natural that medical writers should have been, as they always have been, and as they still are, fond of classifying the several departments of their science and of generalizing its principles. In proportion as a science approaches the positive and perfect, does it naturally and necessarily resolve itself into clear and well established laws; its several parts assume their appropriate places and relations, as the atoms of a salt in solution do in the process of crystallization. Beside the influence of this natural connexion between arrangement, order, classification on the one hand, and the advanced state and certainty of science on the other, there is, to all minds, a seductive charm in the simplicity of broad generalizations. From this source, more than from any other, probably, arose the wide-spread and long-continued popularity of the Brunonian doctrine; and it may, we trust, be safely enough intimated that this same cause has had no very subordinate agency in the rapid propagation of a certain favourite system of pathology in our own day. The many headed monster, NOSOLOGY, was the legitimate offspring of this spirit, a spirit still vigorous and prolific, although that hydra specimen of her progeny has long since been gathered to the capacious but crowded sepulchre of its multitudinous kindred. Dr. Dunglison arranges the articles of the *materia medica*, exclusive of such as act mechanically or chemically, in two great classes. The articles in one of these classes increase, locally or generally, vital action; those in the other diminish it. A third class is added by other modern systematists, to wit: the revulsive. Now these arrangements may be plausible enough, but what claim have they to the positive and demonstrable attributes characteristic of true science? Dr. Dunglison places nauseants in his class of sedatives, and emetics in his class of excitants. By what therapeutical jugglery is tartarized antimony spirited alternately backwards and forward, through or over the broad and high wall of partition which separates the two classes? It is not our object now, however, to comment particularly on the arrangement of Dr. D. It is the purpose of these remarks to show that this entire spirit of general classification is unfriendly, so far as it goes, to a close, discriminating study of the *peculiar* properties of the separate articles of the *materia medica*; and that the great object of our therapeutical researches, on this particular point, ought to be to ascertain the exact individual *specific* value of these separate articles. *No single article of the materia medica,*

which is not endowed with peculiar properties, clearly defined, and, on this account, having, or capable of having, an especial relation to certain morbid conditions, ought to be retained in the list of remedial agents. Tartarized antimony and ipecacuanha are, both of them, emetics; but, it will hardly be pretended that their operation on the stomach, and, through this organ, on the entire system, is identical in its nature, differing only in activity. Each has its peculiar action. Similar remarks may be made in regard to the other important articles of the *materia medica*; and any generalization, the tendency of which is to merge these peculiar properties in some common family character, is unfriendly to the progress and certainty of therapeutical science. This spirit of generalizing, which we are endeavouring to combat, is objectionable not merely on the grounds of its abstract or scientific falsity, if we may so speak. If this were all, we could easily let it pass unnoticed. But this is not all. The error is full of practical danger. It influences directly and unavoidably our whole method and conduct in the selection and application of remedies. Imbue a man thoroughly with this spirit and he will be little careful in nice discrimination. An emetic with him will be simply an emetic; he has an indication to fulfil, and it will matter little what the particular article is which he uses. We think that the practice of the ultra disciples of some modern systems of medicine may be *invoked*, as Dr. D. says, in illustration of these remarks. In the first place, a simple, positive system of pathology is assumed. In the next, a classification of remedial agents is made, which exactly dovetails with this system. The indication is deduced from the pathology, and then any article which happens to be found in the proper category is used to fulfil the indication.

It would be easy to extend to a much greater length our observations on the foregoing subjects. We are well aware that some of the principles which they involve need fuller developement; but the space that we have already devoted to them admonishes to leave them for the present. We have been free in our animadversions, because the pages of Dr. Dunglison's book are overlaid and pervaded with the ideas and doctrines to which these remarks are opposed; and because we think the doctrines themselves essentially wrong in their philosophy, and in their practical tendencies unfavourable to the advancement of therapeutical science.

After having disposed of his preliminary generalities, of his great principles, on the soundness and value of which we have now expressed ourselves with the freedom which the interests of truth always require, and, we trust also, with the candour and courtesy which the same interests no less imperiously demand, Dr. D. enters more immediately the field of therapeutics. We cannot go regularly through all the successive chapters and sections of this portion of the work, for the purpose either of criticism or analysis. As, however, we have thus far had the floor pretty much to ourselves, and lest we should be accused of unfairness in not allowing the author to be heard in his own behalf; and for the more grateful and agreeable purpose, further-

more, of commending to the attention of our readers some of Dr. Dunglison's views on matters of great practical moment, of every day importance, we shall make up, for the most part, the remainder of our notice with such extracts as may best illustrate the general character and composition of the work, and with such, also, as may be practically interesting and valuable to our readers themselves.

The first section treats of excitants proper,—“agents that increase the organic actions by impressing the contractility of the part to which they are applied; the excitation thus induced being extended, or not, to the rest of the system.” We quote from this section the author's remarks on the use of excitants in the treatment of what he calls, loosely enough, we think, *fevers*. This point of practice is certainly an important one—one that has for a long time occupied the attention of physicians and been the occasion of much controversy. Whether the practitioner will find any of his doubts removed, or his principles of action rendered more intelligible and explicit by the following directions, we must leave him to judge.

“It need scarcely be said, that the use of excitants must be invoked with extreme caution in fevers. At one time—as I have already remarked—the great indication, in these affections, was supposed to be,—to obviate the tendency to debility and death, and, accordingly, antiphlogistics, especially of the depleting kind, were used with extreme caution, and every thing was done to husband the strength so as to permit the patient to bear up in the last stages. A better system of medical philosophy fortunately now prevails, and it is universally admitted, that few, if any, die from febrile debility, and that the fatal influence is seated in the over-irritation of some tissue, under which the patient gradually succumbs.

“The efforts of the practitioner are, therefore, properly directed to the prevention of irregular action in organs, and to the removal of irritation or inflammation, wherever existent, and, under this philosophical treatment of fever, excitants are, of course, never employed during the early periods, and it is only when the powers of life begin to flag, that a question arises as to the propriety of their adoption. Even in the very lowest stages of the worst grades of typhus, this question is not always very easily settled. There is generally more or less local irritation present—often in the lining membrane of the stomach and intestines—and many of the signs of debility are dependent upon the depressing influence exerted on other functions by the predominance of irritation there.

“We often observe this depressing influence singularly evinced in the sanguiferous system in diseases of the intestinal canal, especially such as affect the lining membrane. My friend, Professor Smith, of the University of Maryland, and myself attended, some time ago, one of the students of the university, who, after having been present at the lecture of the Professor of Obstetrics, in the evening, was attacked with violent vomiting, but without any abdominal tenderness or other uneasiness. On the following morning there was some slight tenderness on pressure, and the vomiting persisted. He was cupped over the abdomen, although neither the state of the pulse, skin, nor the other symptoms appeared to indicate inflammatory action. During the day, he gradually sank, and expired the same evening. On examining the body, a portion of the ileum was found contracted for the space of several inches, but this contraction must have been forming gradually. The lining membrane exhibited but slight signs of irritation; yet the cause of death was, doubtless, seated in this portion of the economy; and the only way we can account for it, is by in-

voking the intimate and extensive sympathy which exists between this part of the frame and the great centre of the sanguiferous system, so that a slight irritation there may produce marked depression, and even arrestation of the circulatory function. There is something, however, extremely unaccountable in these cases. The peristole of the digestive tube is but indirectly influenced by the brain and spinal marrow. Its functions appear to be principally carried on under the influence of the ganglionic nerves. The heart itself is equally abstracted from direct cerebro-spinal influence, and, indeed, from almost all nervous influence;* yet, in enteritic irritation, we find the whole circulatory apparatus oppressed, as it were; and this oppression, if not removed, rapidly terminating in depression; whilst, in the comparatively harmless disease—*tonsillitis*, or inflammatory sore throat—the action of the heart is inordinately excited, and the whole vascular system is thrown into violent agitation.

"It is generally considered proper to have recourse to excitants in fever, when the pulse becomes feeble and fluttering; the tongue moist perhaps, but with a dark fur; the teeth covered with sordes; the skin bathed in a cold, clammy sweat; or, if hot and dry, with concomitant symptoms of debility; with sinking down in the bed, and low muttering delirium; the tongue tremulous, and protruded with difficulty,—indicating great debility of the nervous system; petechiæ or vibices, produced by transudation of the blood through the loosened parietes of the vessels, &c. &c. But it is impossible to lay down any positive rules for the guidance of the practitioner, and it is better, that he should even allow the signs of prostration to become marked, before he passes to the too early use—as it may prove to have been—of excitants. Dr. Rush, as I have before remarked, considered, that there was a period in fevers, at which blisters might be applied as stimulants with great advantage; but, if used before this period they would be productive of mischief. It is obviously, however, impossible to fix upon any such point with accuracy; and in this, indeed, the main difficulty rests. If it could be decided on by any specific signs, it would be but necessary to apply the antiphlogistic or the stimulating medication accordingly. Vesicants are, however, by no means the best agents to be employed as excitants. In the low conditions of the frame, in which they are conceived to be indicated, the discharge of a large quantity of the serous part of the blood cannot fail to add to the debility more than the excitant property can detract from it; whilst they produce excessive irritation, and are, withal, transient in their operation. A more permanent excitant is, therefore, better adapted to these cases; and internal stimulants—as wine—are preferred, the quantity being carefully regulated so as not to stimulate beyond the due degree.

"Under another head, we shall see, that epispastics may be employed with decided advantage in fever, but not with the view of inducing general excitation.

"Whenever stimulants are esteemed necessary in fever, the fact before adverted to must be borne in mind, that their operation is apt to be followed by corresponding depression. They should be administered, consequently, so frequently, that the depression has not time to intervene, care being taken, that they are not given in such doses as to excite beyond the proper point; and, if their operation be salutary, they will be found to detract from, rather than add to, the febrile irritation; if, however, the febrile symptoms should be manifestly increased under their administration, they must be discontinued—but discontinued gradually—for the reasons just mentioned."

We do not wish to be captious, but we should like to know what would be thought of the practitioner who, when he found the febrile excitement of his typhus or his scarlet fever patient manifestly in-

* 'Human Physiology,' First Edit. ii. 133, and Second Edit. ii. 144.

creased under the administration of stimulating medicines, should be careful to discontinue them *gradually*. Is it necessary that we should call the notice of the attentive reader and the cautious reasoner to the case of the student? Was there ever an instance of more utterly gratuitous assumption, of more naked and wide conjecture, than that which attributes the sudden death in this instance to the *gradually formed* contraction of a portion of the ileum, the lining membrane of which exhibited but slight signs of irritation?

Passing over the two sections on tonics and anthelmintics, we come next to the class of astringents. We have nothing to say of this section, except to offer a remark or two on the confidence with which the author speaks of the uselessness of all astringent remedies, unless they can be brought into direct contact with the surface from which the preternatural discharge takes place. "It is manifest," he says, "that in all increased discharges which occur from parts that can only be reached through the medium of the circulation, no signal advantage can be expected from the administration of astringents." And the reason why we are to expect no signal advantage is this, not that experience forbids us to do so, for the language of experience is quite the other way, but because we cannot see, or rather because Dr. Dunghlison cannot see, *why* any advantage *should* be looked for. The astringent substance, if introduced through the medium of the circulation, must come in contact with the diseased surface—that of the uterus or urethra, for example, even if it reaches this surface at all, so changed and diluted that it cannot produce any considerable effect upon it; or rather we do not see *how* it can, and *therefore* it does not. No appeal or reference whatever is made to the teachings of experience. We are told by a man sustaining the high character of a practical physician, and performing the responsible functions of an authoritative teacher from the chair and through the press, that sugar of lead or sulphate of copper can be of little or no use in arresting or diminishing excessive discharges from any surface to which it is not immediately applied, *because* it is difficult for a *rational* therapist to see *how* or *why* it should be of any use. If this sort of idolatry in medicine—this worshipping of the false gods which our own hands have fashioned—affected matters of abstract faith only, we should be well content, as we have already said, to let it pass unrebuked; but when it comes to influence, so seriously and vitally, our works as well as our belief, it is quite essential to our salvation, or rather to that of our patients, that the false doctrine should be exposed.

The two following sections are devoted to emetics and cathartics. We should leave the second, as we do the first, without comment, did we not feel ourselves bound, in conscience and duty, to notice the following very extraordinary assertion;—"In the various head affections, and especially in encephalitis,—whether involving the brain or its membranes, or both—cathartics would clearly be advantageous, by virtue of the revulsion they effect, did not the inconveniences, to which the patient is subjected by the motion necessarily attendant on their operation, often preclude their employment." If

we thought it within the limits of possibility, that a single regularly educated practitioner,—that any one who will be likely to read the book before us,—however young or inexperienced, should be deterred from a practice, the necessity of which is so well settled and so unanimously admitted as that of free and active purging “in the various head affections, and especially in encephalitis,—whether involving the brain or its membranes, or both,”—by this paragraph of our author’s, we would give a moment to the exposition of its dangerous errors. But this possibility can hardly exist, and we leave the sentence and the section without further comment.

We pass over the sections on emmenagogues, diaphoretics, errhines, diuretics, expectorants and sorbefacients. The last mentioned class consists of medicines which promote absorption. Among other agencies endowed with this property, the author classes “mental” sorbefacients. “Of these,” he says, “we have examples in the effect of the imagination in discussing tumours of various kinds. Some of these growths are possessed of but little vitality; and if the nervous and vascular influence be detracted from them, they speedily die. *This is the way in which charms remove warts*. It is a common popular superstition, that if we steal a piece of beef, rub warts with it, then bury the beef, or throw it over the left shoulder without looking behind us, as the beef rots the warts will decay; and that a dead man’s hand, rubbed nine times on a wen or an enlarged gland, will dispel it; and such is the occasional result.” The removal of warts by such a process as the stealing of beef, rubbing the pathological part with the stolen article and then burying it, or throwing it over the shoulder without looking behind us; or by the simple, more direct, and withal the honest one, of selling these verrucous excrescences, by a fair business transaction, to some accommodating neighbour who is willing to purchase, is thus philosophically accounted for, and fairly installed within the domain of scientific therapeutics.

The next section, devoted to revellents or derivatives, seems to us to be a better one than any of its predecessors. We shall do ourselves the pleasure, our author the justice, and our readers the profit of quoting some of Dr. Dunglison’s observations of a practical nature from this section. They are sensible and judicious.

“The inflammation of the skin, caused by vesicants, is occasionally attended with fatal consequences. It is of the erysipelatous kind, and, under particular circumstances—as regards age, condition of the system, &c.—the inflammation eventuates in gangrene and death. In very young children, great irritation is apt to be induced by blisters, and, if the child be labouring under any morbid condition of the dermoid tissue,—such, for example, as is present in measles or scarlatina, the inflammation may terminate unhappily. To obviate this, when vesicants are esteemed necessary in the diseases of infants, they should not be permitted to remain too long on the part. From four to six hours will be sufficient, and a piece of fine gauze or of tissue paper may be placed between the blistering plaster and the skin—if cantharides be used—in order that no particle of the flies may adhere to the vesicated surface. We can hardly imagine an occurrence more disagreeable to the philanthropist than that of a patient dying, in consequence of the application of an agent from which he expects a cure, or at least a mitigation of the symptoms; great caution is there-

fore necessary in the use of these agents, in very early life, especially in the diseases referred to. I have known three or four cases of death manifestly accruing from the use of blisters under such circumstances, although it is probable, that in most of the cases a fatal event might have ultimately resulted, from the disorganization, produced by the mischief for which the blister was recommended.

"There is another great inconvenience, attendant upon the employment of vesicants composed of cantharides. This is—the absorption of the cantharidine, which enters the circulation, and proceeds to the urinary organs, giving rise to strangury, and, at times, to intense vesical irritation. That this is the mode in which the effect is produced,—that is, by entering the circulation,—is demonstrated by the fact, that the intervention of tissue paper, or of gauze, although it may not prevent vesication, effectually obviates the strangury;—the tissue paper preventing the absorption of the cantharides, which would otherwise have been effected. Yet, some have referred the strangury, in such case, to sympathy. Were this explanation correct, the tissue paper or gauze ought not to prevent it, as the vesication is accomplished through them. At times, it becomes necessary to apply the blistering plaster over a surface, which has been scarified in the operation of cupping. The only precaution, here again, requisite, is to cover the wounds, made by the scarificator, with tissue paper."

"With regard to the question—whether a permanent or an intermittent revulsion is more efficacious?—I do not think that much dissidence now exists amongst therapeutists. The majority are unequivocally in favour of the latter plan, although circumstances may often induce them *practically* to have recourse to the former. The reasons, in favour of this preference, are cogent. When an artificial irritation, accompanied, or not, with increased secretion from the part, has been established for some time, it ceases, in a great measure, to be a morbid condition, and becomes, as it were, a part of the healthy function, so that it cannot be arrested, without inconvenience being apt to result, and without danger of a centre of fluxion being established in some internal organ, more disposed than others to assume the morbid condition. In this way, many discharges, the result of morbid action, become, in process of time, healthy, and cannot be officiously interfered with. On the other hand, if a succession of irritations be produced, the system never becomes habituated to them, and the repetition of the irritation, after the lapse of a short period, occasions the same beneficial impression, as on its first employment. Hence it is, that a succession of vesicants, and, indeed, of every variety of epispastic, is to be preferred to a more permanent application—and that issues and setons lose much of their beneficial influence in the latter periods of their employment;—their good effects, as revulsives, being in an inverse ratio with the shortness of the period, during which they have been in action.

"The intensity of the artificial irritation, induced by a revulsive, is worthy of consideration, in a therapeutical point of view. If it be but trifling, it may be insufficient to break in upon the internal morbid catenation; and, on the other hand, if too violent, irritative irradiations may pass in various directions, and may even add to the internal mischief. Every practitioner must have occasionally witnessed an aggravation of symptoms from this cause, especially in those whose nerves are unusually impressible. In such, no variety of epispastic can, at times, be used. I have, at this time, under my care, a lady, who is thrown into the most violent nervous agitation, by the application of the smallest blister; and blisters have occasionally been known to induce convulsions. There are certain individuals, too, who suffer excessively from the vesication induced by cantharides, and yet, who are not—what would be called—*extremely nervous*. Their cutaneous nerves are, however, inordinately impressible. In such persons, vesicants would necessarily fail in their effects—by the general disorder, which would follow the high degree of erethism of the

dermoid structure. In such individuals, blisters are never found to exert their salutiferous agency; on the contrary, the irritation, they produce, is reflected to every part of the economy, and too often the diseased action, for the removal of which they were applied, is, in this way, augmented. In like manner, where the powers of the system have been greatly reduced, and much nervous irritability has been developed, blisters are apt to produce mischief. Broussais has made this fact the foundation of one of his propositions,—restricting it, however, to cases of gastro-enteritis. ‘Vesicatories,’ he says, ‘often augment gastro-enteritis, because the inflammation, which they produce, adds to that of the digestive mucous membrane, instead of producing revulsion; they do not, then, render the services expected from them, in that grade of these diseases, which is called “adynamic fever.”’*

“The character, and period of the disease have much to do with the action of revellents. Every practitioner must have observed, that in the phlegmasiæ, when the disordered actions run inordinately high, but little effect is usually produced by revellents. I am not, however, prepared to say, that they invariably do harm. I had an opportunity of witnessing, for a long time, the practice of an individual, much engaged with the duties of his profession, who always had recourse to blisters, from the first onset of inflammatory affections, often not only with impunity, but with manifest advantage. Theoretically, it would seem, that if the manifestations are already largely exalted, any source of irritation ought to add to such exaltation; the principle, however, that two irritations do not easily exist, at the same time, in the body, to the like intensity, applies even here; and, I can say, from extensive observation, that although the beneficial agency of revellents is not as marked as where remedies have been premised to allay the tumult in some measure, they have not always appeared to me to aggravate the disease, and have often been followed by a mitigation of the morbid action. I cannot, therefore, subscribe to the opinion, that “in very strong subjects affected with intense irritations, accompanied with considerable febrile excitement, and having their seat in viscera important to life, or propagated to large surfaces, revulsion is next to impossible, and cannot even be attempted without danger.”† The danger does not appear to me to rest so much on the employment of the revulsives, as on the neglect of more effective measures, which such cases imperiously demand.”

The recommendation contained in the following extract is new to us. Its value must be settled by experience.

“In the case which I before offered—of encephalitis liable to be augmented by epispastics applied to the shaved scalp, we rarely observe, in American or British practice, the revulsive applied at a greater distance than to the nape of the neck. There is no reason, however, why this part should be preferred to the top of the sternum,—where I am in the constant habit of applying it in such cases, and, as I conceive, with full advantage, whilst there is an overwhelming objection to the former situation,—in the circumstance of its rendering the position on the back uncomfortable, and, at times, impracticable, owing to the abraded surface being subjected to attrition on every change of position of the head. Practitioners have been so little in the habit of reflecting on the mode in which epispastics produce their salutary agency, in these cases, that they rarely select a more convenient, and yet equally effective, centre of irritation, at a greater distance from the seat of the mischief. Many of the French writers recommend blisters to be placed upon the arm, or between the shoulders, in encephalitis; but it has always appeared to me, that the top of the sternum is perhaps the least objectionable situation, where counter-irritants are demanded in head affections, accompanied by exaltation of the organic actions.”

* Prop. 288.

† Begin, *Op. citat.* p. 747.

The whole section from which these selections are made is above the average standard of the work of which it forms a part. It is less incumbered with faulty pathology and fanciful explanations, and is marked throughout by the better spirit of cautious observation.

Omitting any notice of the section on antispasmodics, we come next to the chapter on sedatives. Dr. Dunglison very properly, we think, admits without any hesitation the existence of "agents that directly depress the vital forces." Among these he places hydrocyanic acid, hydrosulphuric acid, sulphuretted hydrogen, some other of the gases, and tobacco. This list might probably be considerably extended. In this chapter our author enters somewhat largely into a general discussion of the uses and abuses of blood-letting. Many of his remarks on these important topics are characterized by much good sense and discretion. Dr. Dunglison is a cautious, discriminating bleeder; the disciples of Bouillaud in Paris and the pupils of the Sangrado, Gallup school in New England, would call him a timid and temporizing one. We give below some of his views and opinions, leaving our readers to choose, on such evidence as their reading and experience may furnish them, between the moderate and careful practice of our author and that of the more fearless and ultra advocates of the lancet.

After having spoken of a case where symptoms of acute cerebral inflammation occurred in an irritable and nervous female, Dr. D. says:

"I am satisfied, that had depletion been carried to a greater extent in this case—as it would most certainly have been, by those practitioners, who believe that general blood-letting is the only agent, that can be relied on as an anti-phlogistic, great mischief would have resulted. Long, indeed, before Dr. Marshall Hall had published his interesting 'Researches on the effects of loss of blood,' I had been deeply impressed with what appeared to me the faulty views, entertained, both as regards the pathology and the therapeutics of such cases as those I have mentioned; and had satisfied myself, that the maxim inculcated by many practitioners as applicable to internal inflammations in general—'when you have bled in inflammation to such an extent, that you are doubtful, owing to the persistence of the symptoms, whether you should bleed again,—bleed,'—was a faulty one, and often, I fear, attended with disastrous consequences. As a general rule, I would say, on such occasions of doubt and difficulty, 'do not bleed, but have recourse to some other appropriate sedative, or revulsive agent, until your doubt is removed.' Every practitioner, much engaged with the diseases of women, must have met with cases of peritoneal inflammation in the puerperal state, in which, after bleeding has been practised as far as he has esteemed it safe, the effects of a sedative dose of opium have been signally salutary. The irritability of the nervous system has been allayed by it; whilst, if the bleeding had been repeated, it might have been formidably developed."

After quoting from Gooch, Dr. Marshall Hall and Abercrombie, their accounts of a morbid condition of the brain, occurring most commonly in children, where most of the usual symptoms of cerebral inflammation or congestion seem to be dependent upon an opposite pathological state of this organ, Dr. D. goes on to say:

"I have cited the above remarks at some length, in consequence of their marked accordance with the views I have been led to entertain in what have appeared to me to be similar pathological conditions; and I am satisfied, that

both in children and adults, we often meet with an analogous state of the brain, and especially in scarlatina. In the disturbed state of the encephalic functions, which so often attends that anomalous disease, we recognise—it has appeared to me—a condition very different from that, which is produced by active inflammation or congestion in the encephalon. Under the great expenditure of nervous energy, which takes place over the whole of the capillary surface, and which is indicated by the inordinate activity of the agents of calorification, of which the organic nerves distributed to the capillary blood-vessels are the most important, the great encephalo-spinal centre appears at times to be in a state very different from that of inflammation or active congestion. It is rather exhausted by the unwonted energy of the organic portion of the nervous system; and, accordingly, in many such cases, the use of diffusible excitants has been found serviceable,—the delirium or the coma gradually disappearing as the system begins to feel their compensating influence. This practice has been adopted in scarlatina, when accompanied by such signs of encephalic disorder, with great success, by my friend, Dr. Baer, of Baltimore, and it has been followed by the most happy results in one or two cases, which fell under my own care. Under the vigorous use of depletives, the symptoms have not been mitigated; at times, indeed, they have seemed to be aggravated; but on changing the system of treatment, and having recourse to tonics or excitants, a marked amelioration has speedily ensued.”

In regard to blood-letting in the convulsions of children, our author makes the following remarks:

“Prior to the period of the first dentition, owing to certain evolutions of organs, the nervous system is, as we have previously seen, unusually impressible, so that sources of intense irritation, existing any where, may be the occasion of irritative irradiations proceeding in all directions, until the parts of the cerebro-spinal axis have their functions deranged, and sensation, volition, and mental and moral manifestations become, for the time, suspended. In this manner, the irritation produced by the pressure of a tooth against the gum, or any source of excitation in the intestinal canal may become the cause of convulsions; and, after the functions of the cerebro-spinal axis have been once so far deranged as they are during convulsions, they are extremely prone to reassume the morbid condition, until, ultimately, organic disease of the encephalon occurs, or the little sufferer is worn out by continued irritation. Now, in such a case, the predisposition to the disease is—the period of life; and the exciting cause is—the irritation in the alimentary tube. Great mobility of the nervous system—great impressibility—is present even in health, and this impressibility only requires the application of a sufficient exciting cause to have convulsions developed.

“In addition to this general predisposition, derived from time of life, there is doubtless an organization, obtained from progenitors, which predisposes to convulsions. We frequently see a whole family subject to them, during childhood; and, on inquiry, we may find, that one of the parents was liable to the disease in his childhood. In such cases, a less energetic exciting cause, is required to develop the mischief, possessing, as they do, a double source of predisposition. In the cases which I am now describing, we cannot invoke polyæmia, or hyperæmia of the encephalon. They are wholly neuropathic. The predisposition is unusual nervous impressibility; the exciting cause is often situated in the digestive tube; and very frequently the irritation there is produced by food of an improper character, or by some inflammatory or other morbid condition of the mucous or lining membrane. The indication cannot, consequently, consist in diminishing the quantity of blood circulating in the system, with the view of removing any supposititious congestion of the brain. Blood-letting, indeed, in such a state, could hardly fail to add to the mobility and impressibility of

the nervous system, and it has appeared to me, in many cases, to have been followed, too manifestly, by augmentation of the symptoms. The convulsions have recurred; the surface has become cool, and pale,—almost exanguinous; the circulatory forces have exhibited, that their action was enfeebled; the little sufferer has continued in a state of coma between the fits, or has had but short intervals of consciousness; and he has gradually sunk with no signs of hyperæmia,—unless we consider the convulsions and the coma to indicate such a condition—for, on dissection, no morbid appearances whatever have been met with in the brain, or that effusion of serum has been discoverable, which, as I have before shewn, is present, when we bleed an animal—and a healthy animal—to death.

“Proceeding on those pathological principles, I have not often considered it proper to abstract blood in the convulsions of infancy; in almost all cases, it has been but necessary to clear the alimentary tube by a gentle emetic, followed by a mild cathartic; to keep the child from every source of irritation, that could act injuriously on his organs of sensation from without, or on the intestinal mucous membrane from within; to equalize, as far as practicable, the excitability of the cutaneous surface by the use of friction or of the warm bath; and, under this plan of management, I have almost always found the affection eventuate favourably. At the same time, it is proper to remark, that there are cases of convulsions accompanied by every sign of vascular excitement, and where a true polyæmic or hyperæmic condition of the brain exists. Here, of course, blood-letting is the main agent to be relied on. If encephalitis be present, it must be treated as encephalitis; but, in all cases, the most careful attention must be paid to discriminate, whether the convulsions are accompanied or produced by a redundancy, or by a deficiency of nervous and vascular energy.”

Dr. Dunglison's cautiousness on this important point of therapeutics is very manifest in the following extract:

“The extent to which blood-letting should be carried, in cases of violent internal inflammation, is often a matter of great difficulty with the discriminating, but of no difficulty whatever with the reckless and uninformed. In his state of blissful ignorance, the latter continues to bleed, and consoles himself, when the fatal result has been hastened—perhaps mainly induced—by his agency, that the sufferer has fallen a victim to an incurable malady. Many have laid down a rule, before referred to, that when we have pushed the blood-letting to such an extent, and so often, that we are in doubt whether the operation should be repeated, the decision should be in the affirmative. But, with the disposition, which prevails so generally,—and which prevailed, a few years ago, to a much greater extent than it does even at present,—to bleed without due consideration, such a doubt will rarely be felt, without good ground at the same time existing for staying the hand; and, therefore, the decision, according to my experience, ought generally, as I before said, to be in the negative. The argument commonly urged for the farther abstraction of blood is, that the inflammation manifestly persists, and that it must inevitably destroy if not arrested;—that blood-letting is more likely to subdue it than any other therapeutical agent; and that if it should not, the physician will have the consolation of knowing, that he has done every thing in his power to avert the melancholy termination. Were the abstraction of blood, in all cases and to any extent, devoid of danger, this mode of viewing the subject might be logical; but mischiefs result from bleeding, in these and similar cases, which are fairly referable to the operation, and are equally serious in their results with the disease for which it may have been employed.”

We do not think our author's anecdotes and illustrations, historical

and poetical, always the most near-fetched and opportune, but the following appears to us worth at least the small space that it will occupy and the little time required to read it.

"The satisfaction, often felt at the exhibition of energy on the part of the practitioner, is well exemplified by an anecdote, which an illustrious native of this country—now no more—who had filled the highest office in the gift of a free people, and whom I had the honour of ranking amongst my personal friends—was in the habit of recounting.

"Travelling from Virginia towards the north, he rested for the night at a tavern on the road; soon after his arrival at which, the hostess came in from a neighbouring house, with the females of her family,—all exhibiting marks of deep distress. He was informed that they had been witnessing the parting scene of a young friend, who had died of some acute affection. 'But, thank God!' observed the contented matron, 'every thing was done for him that was possible, for *he was bled seven-and-twenty times.*' 'It is not'—says the inimitable Molière, who was unsparing in his appropriate philippics against the profession, and the public of his day—'it is not, that, after all, your daughter may not die; but, at all events, you will have done something and you will have the consolation, that she died according to form.' ""*

There are many other portions of this valuable chapter which it would gratify us to copy, but the extent to which we have already carried our extracts, and the length also which our notice is attaining, both admonish us to desist. We shall finish our quotations, therefore, with the following observations relating to the application of leeches to young children. We presume that there are few of our readers, who have resorted much to this very valuable means of treating inflammatory disease in young subjects, who have not been summoned, more than once, to their little patients, cold, panting and waxen-hued from the loss of blood by two or three leech bites.

"It has fallen to my lot to witness some alarming cases of exhaustion, especially in children, where leeches have been applied: in two cases indeed the result was fatal. In both the cases due attention had not been paid, and a large amount of blood was lost before the cause of the sinking was discovered; and in one of them every attempt to arrest the flow of blood failed. These cases are rare, but they constitute objections to the use of leeches which do not apply to cupping,—the flow from the wounds made by the scarificator being readily arrested. When practicable, the leeches should be placed over bone, in order that pressure may be conveniently made on the bleeding vessels, should such a course be requisite.

" 'When leeches are applied to soft parts,'—says an author to whom I have often referred,—'for instance, to the abdomen, it is truly astonishing how much blood sometimes is detracted; particularly when a poultice is applied over the bites, and the patient is kept warm in bed: to prevent, therefore, injurious symptoms of exhaustion from such a circumstance, the poultice should be frequently examined. This is more likely to occur in children than in adults; and in children it not unfrequently happens that the bleeding cannot be stopped without encircling the orifice with a ligature. On this account leeches should never be applied late at night on children; for, as the application of leeches in infancy must be regarded as a species of general blood-letting, the precise num-

* "Ce n'est pas qu'avec tout cela votre fille ne puisse mourir, mais au moins vous aurez fait quelque chose, et vous aurez la consolation, qu'elle sera morte dans les formes."—L'Amour Médecin, Act. i. Scene v.

ber which will regulate not only the quantity, but be equivalent to rapidity in the detraction of the blood should be determined; but the bites should be instantly closed, on observing that the system is brought under the influence of loss of blood.' **

The remainder of the work treats of the following classes of medicines, to wit: narcotics, refrigerants, nauseants, antacids, antalkalies, antilithics, disinfectants, antiseptics, demulcents, diluents, and alteratives. The last chapter is one of "concluding observations;" and this, together with the volume, is terminated by some excellent passages from an excellent essay by Professor Bigelow of Boston—an essay as unlike in its spirit and character the work to which it is here appended as can well be imagined.

We have spoken of Dr. Dunglison's treatise with freedom: it is very probable that the author and his friends may think that we have spoken, in some instances, with undue license and unnecessary severity. We have not intended to do so. We belong to no party, medico-doctrinal or medico-geographical; we have no personal interests, prejudices or passions, to be in any way affected, or that can in any degree have influenced our feelings or our language. We have said what the interests of truth, science and humanity seemed to us to require, and no more. We are advocates for free discussion, in its best and fullest sense; but we believe that no discussion can be truly free which is not also courteous in manner, and as kind in spirit as the circumstances will allow. We believe the doctrines we have controverted to be dangerous and erroneous, but we distrust with too much sincerity our own opinions, as well as those of other men, to claim for them any exemption from fallibility. We ask for them only that fair hearing and attention which the deliberately formed opinions of any earnest lover of science and humanity may always reasonably claim.

E. B.

ART. XII. *A Treatise on the Malformation, Injuries and Diseases of the Rectum and Anus.* By GEORGE BUSHE, M. D., &c. New York: French & Adlard, 1837. 1 vol. 12mo., pp. 299, with nine quarto plates.

The author of this little treatise has been cut off in the midst of his labours since the completion of the work; the lights of his experience and reflections will never again be thrown before the public, and one of the chief purposes of a critical review, the correction of the peculiar faults of an author, is no longer capable of fulfilment. It only remains for us to notice the claims of the essay upon the attention of the profession.

The range of subjects discussed by Dr. Bushe, is nearly as exten-

* Thomson, op. cit. i. p. 458.